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CONTROVERSIES IN THE HISTORY OF THE RADIATION REACTION PROBLEM IN GENERAL RELATIVITY

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1 Introduction

Beginning in the early 1950s, experts in the theory of general relativity debated vigorously whether the theory predicted the emission of gravitational radiation from binary star systems. For a time, doubts also arose on whether gravitational waves could carry any energy. Since radiation phenomena have played a key role in the development of 20th century field theories, it is the main purpose of this paper to examine the reasons for the growth of scepticism regarding radiation in the case of the gravitational field. Although the focus is on the period from the mid-1930s to about 1960, when the modern study of gravitational waves was developing, some attention is also paid to the more recent and unexpected emergence of experimental data on gravitational waves which considerably sharpened the debate on certain controversial aspects of the theory of gravity waves. I analyze the use of the earlier history as a rhetorical device in review papers written by protagonists of the "quadrupole formula controversy" in the late 1970s and early 1980s. I argue that relativists displayed a lively interest in the historical background to the problem and exploited their knowledge of the literature to justify their own work and their assessment of the contemporary state of the subject. This illuminates the role of a scientific field's sense of its own history as a mediator in scientific controversy.[1]

2 The Einstein-Rosen Paper

In a letter to to his friend Max Born, probably written sometime during 1936, Albert Einstein reported

> Together with a young collaborator, I arrived at the interesting result
>
> that gravitational waves do not exist, though they had been assumed a

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certainty to the first approximation. This shows that the non-linear general

relativistic field equations can tell us more or, rather, limit us more than

we have believed up to now. (Born 1971, p. 125)

The young collaborator was Nathan Rosen, with whom Einstein had been working for some time, producing papers on several topics. They had submitted a paper to the *Physical Review* based on the work referred to in Einstein's letter to Born under the title "Do Gravitational Waves Exist?"[2] and the answer they proposed to give, as the letter states, was no. It is remarkable that at this stage in his career, Einstein was prepared to believe that gravitational waves did not exist, all the more so because he had made them one of the first predictions of his theory of general relativity. In his autobiography Leopold Infeld, who arrived in Princeton in 1936 to begin an important collaboration with Einstein, described his surprise on hearing of the result (Infeld 1941, pg. 239). Despite his initial scepticism, Infeld soon allowed himself to be convinced by Einstein's arguments, and even came up with his own version of the proof, which reinforced his belief in the result (Infeld 1941, pg. 243). However, not everyone was so easily convinced. When Einstein sent the paper to the *Physical Review* for publication, it was returned to him with a critical referee's report (EA 19-090), accompanied by the editor's mild request that he "would be glad to have your reaction to the various comments and criticisms the referee has made." (John T. Tate to Einstein July 23, 1936, EA 19-088). Instead, Einstein wrote back in high dudgeon, withdrawing the paper, and dismissing out of hand the referee's comments (Einstein to Tate July 27, 1936, EA 19-086):

Dear Sir,

We (Mr. Rosen and I) had sent you our manuscript for

publication and had not authorized you to show it to

specialists before it is printed. I see no reason to address the

- in any case erroneous - comments of your anonymous expert. On the

basis of this incident I prefer to publish the paper elsewhere.

respectfully,

P.S. Mr. Rosen, who has left for the Soviet Union, has authorized

me to represent him in this matter.[3]

To this Tate replied that he regretted Einstein's decision to withdraw the paper, but stated that he would not set aside the journal's review procedure. In particular, he "could not accept for publication in THE PHYSICAL REVIEW a paper which the author was unwilling I should show to our Editorial Board before publication." (Tate to

Einstein July 30, 1936, EA 19-089). Einstein must have continued in his dislike of the Review's editorial policy (which in fairness may have been unfamiliar to him, the practice of German journals being less fastidious[4]), for he never published there again.[5] The paper with Rosen was, however, subsequently accepted for publication by the Journal of the Franklin Institute in Philadelphia.[6]

What had led Einstein to the conclusion which so surprised Infeld? He and Rosen had set out to find an exact solution to the field equations of general relativity which described plane gravitational waves, and had found themselves unable to do so without introducing singularities into the components of the metric describing the wave. As a result, they felt they could show that no regular periodic wavelike solutions to the equations were possible (Rosen 1937 and 1955). However, in July of 1936, the relativist Howard Percy Robertson returned to Princeton from a sabbatical year in Pasadena and subsequently struck up a friendship with the newly arrived Infeld. He told Infeld that he did not believe Einstein's result, and his scepticism was much less shakeable. Certain that the result was incorrect, he went over Infeld's version of the argument with him, and they discovered an error (Infeld 1941, pg. 241). When this was communicated to Einstein, he quickly concurred and made changes in proof to the paper which was then with the Franklin journal's publisher (Infeld 1941, pg. 244 and letter, Einstein to editor of the Franklin Journal November 13, 1936, EA 20-217).[7]

Although a footnote attached to the published version acknowledges Robertson's help, it does not indicate its nature (Einstein and Rosen 1937). However, it appears that his chief contribution was to observe that the singularity could be avoided by constructing a cylindrical wave solution. In this way the offending singularity would be relegated to the infinitely long central symmetry axis of the wave, where it was less objectionable, being identifiable with a material source (Rosen 1955). In view of this, Einstein might have been better advised not to dismiss the referee's report so hastily, as the anonymous reviewer also observed that, by casting the Einstein-Rosen metric in cylindrical co-ordinates the apparent difficulty with the metric was removed, and it was easily seen to be describing cylindrical waves (Referee's report, EA 19-090, pgs. 2,3,5).[8] That Robertson was familiar with the referee's criticisms is shown by his letter to Tate of February 18, 1937 (Caltech archives, Robertson papers, folder 14.6) in which he says

> You neglected to keep me informed on the paper submitted last summer by your most distinguished contributor. But I shall nevertheless let you in on the subsequent history. It was sent (without even the correction of one or two numerical slips pointed out by your referee) to another journal, and when it came back in galley proofs was completely revised because I had been able to convince him in the meantime that it proved the opposite of what he thought.

You might be interested in looking up an article in the Journal of the Franklin Institute, January 1937, p. 43, and comparing the conclusions reached with your referee's criticisms.

This suggests that, in spite of himself, Einstein did benefit from the referee's advice in the end, by a very circuitous route.

In fact the cylindrical wave solution presented in the revised paper had been previously published by the Austrian physicist Guido Beck in 1925, but his paper has been largely overlooked since. In a 1926 paper by Baldwin and Jeffrey, and in the referee's report on Einstein's paper, there was discussion of the fact that singularities in the metric coefficients are unavoidable when describing plane waves with infinite wave fronts, but although there is some distortion in the wave, "the field itself is flat" at infinity, as the referee noted (EA 19-090, pg. 9). In any case, the Einstein-Rosen paper, as published, contains no direct reference to any other paper whatever. Rosen published a paper in 1937 in a Soviet journal, carrying through what is presumably the chief argument of the original version of the Einstein-Rosen paper, in order to show that plane gravitational waves were an impossibility due to the ineradicability of singularities in the metric. In the immediate post-war period, other papers suggested that plane waves were not permitted in General Relativity (for example, McVittie 1955). Felix Pirani and Hermann Bondi were both partly motivated by these papers to work on the problem of gravitational waves.[9] In the mid-fifties, Ivor Robinson independently rediscovered the plane wave metric and, together with Bondi and Pirani, published the seminal work on the subject. They were familiar with Rosen's paper, and noted that his regularity conditions for the metric were unnecessarily severe by post-war standards. "In effect, Rosen did not distinguish sufficiently between co-ordinate singularities and physical singularities, which could, in principle, be detected experimentally" (Bondi, Pirani and Robinson 1959).[10]

3 Origins of the idea of Gravitational Radiation

In 1916, in a paper exploring the physical implications of the final version of his general theory of relativity, Einstein proposed the existence of gravitational radiation as one of its important consequences (Einstein 1916). Although both Maxwell and Poincaré have been cited as anticipating the idea of gravitational waves (Havas 1979 and Damour 1987), Einstein produced the first concrete description in a relativistic field theory. In a subsequent paper of 1918, Einstein corrected some errors in his previous description of the waves, and went on to calculate the flux of energy carried by the waves far from their source (Einstein 1918). Appealing to the principle of conservation of energy, he assigned an equivalent loss of energy to the source system, an effect already familiar from electromagnetic theory, nowadays known variously as "radiation reaction", "back reaction" or, in cases involving the decay of periodic motion such as orbital motion, "radiation damping". Because Einstein's formula for the energy emission depended on changes in the mass quadrupole moment of the source, it became known as the quadrupole formula. In deriving the formula, Einstein made use of a linearized version of his field equations both for ease of manipulation and because of its strong analogy to the field equations of electromagnetism. Not surprisingly, therefore, his quadrupole formula was itself similar in form to the multipole radiation formulas of electromagnetism, in which field, however, the lowest order of emission is the dipole.

Einstein was not the first to discuss gravitational radiation reaction. In 1908, Poincaré had suggested that planetary orbits must slowly lose energy to wave emission in the gravitational field and showed that any such effect was too small to explain the perihelion shift of Mercury (Poincaré 1908). As early as 1776, Pierre Laplace had considered the problem of an orbital damping force arising from a finite speed of propagation of gravity. His aim was to discover an explanation for the observed decrease of the Moon's orbital period with respect to ancient eclipse observations (Laplace 1776).

In general there are two distinguishable approaches to the back reaction problem. The first, and generally the simpler is the energy balance argument used by Einstein in his 1918 paper. This approach has been criticized in principle on several counts in the context of general relativity, but was an obvious choice for a first approximation. The second approach, more direct but much more complex, is to iteratively calculate the effect of the source's own field (changing because of the source's motion), upon the source's motion, corrections to which can then be reapplied to calculate the field more accurately. This iteration is carried through one or more steps until it is judged that the reaction effects have been calculated to the desired level of accuracy. This problem is part of a more general one known as the problem of motion. Laplace's method, which took into account the deflection of the Newtonian central force on an orbiting body as a result of the time lag in propagation, was a "one-step" calculation of this type. A key issue in this approach is the fact that the field, in the case of finite propagation, is "retarded", which is to say that the field experienced at a given point in space, at a given time is not that produced by the source at that time, but that of the source at an earlier time, where the difference between the two times is the time of propagation of the field changes from the source's retarded position to the field point in question. As Laplace showed, an orbital decay would be one consequence of introducing retarded propagation instead of dealing with instantaneous propagation. His ultimate conclusion, however, was that the lunar orbital decay could be explained by other, conservative gravitational effects. Therefore finite propagation times had no observable effect in real systems, and the instantaneous action-at-a-distance hypothesis of the day was justified (Laplace 1825). [11]

4 Later work on Radiation

Arthur Stanley Eddington is associated with the remark that gravitational waves propagate with "the speed of thought" (Eddington 1922). Despite the scepticism this implies, Eddington was arguing only that certain classes of gravity waves, the "transverse-longitudinal" and "longitudinal-longitudinal" waves analyzed by Weyl (1921) and Einstein (1918) were unphysical. As mere coordinate effects they could be propagated with any velocity desired by the human mind. In the linearized theory at least, Eddington could show that transverse-transverse waves could carry energy, and he reproduced Einstein's quadrupole formula while correcting an erroneous factor of two in Einstein's early version (Eddington 1922, pg. 279). He noted, at the same time, that the linearized theory was invalid for sources such as binary stars, in which the system was held together by gravitational forces (Eddington 1922, pg. 280). In 1941, the Russian physicists Lev Landau and Evgenii Lifschitz published a back reaction calculation which did treat a binary star system, including its gravitational binding, in the slow-motion weak-field case (Landau and Lifschitz 1951). Their analysis has been influential, although some have felt that it took too much for granted, a problem worsened by the book's terse style.

Although the main topic of the Einstein-Rosen paper had nothing explicitly to do with the back reaction problem, it is very noteworthy as the first serious (if abortive) attempt to disprove the existence of gravitational waves. In an interesting passage addressing radiation reaction, the published paper suggests that one is not compelled to the conclusion that waves emitted by a source must damp the source's motion, if one supposes that any outbound radiant energy is matched by a second system of incoming waves, impinging on the source. In short, they observed that the use of half-advanced plus half-retarded potentials will avoid motion damping in the source system even if the waves exist. "This leads to an undamped mechanical process which is embedded in a system of standing waves," in the author's words (Einstein and Rosen 1937). The paper refers cryptically to the work of Ritz and Tetrode "in former years" relating to the question of advanced versus retarded potentials, and it appears that Einstein often quoted Ritz approvingly in this context (Infeld and Plebanski 1960, pg.201).

Walter Ritz, a Swiss contemporary and friend of Einstein's had complained in his criticism of Lorentz's electrodynamics that advanced potentials (in which the field at time t is that produced by the source from a *future* position) were admitted as solutions of the equations of electrodynamics just as well as the retarded potentials (Ritz 1908). To Ritz, this defied the principle of causality, since effect preceded cause. Just as abhorrent to Ritz were combinations of the two potentials, such as the average of advanced and retarded fields (half-advanced plus half-retarded) which allowed "perpetual" motion because, like the instantaneous interaction, it produced no motion damping due to back reaction. Ironically, what Ritz regarded as so damning, Einstein appears to imply might have a positive virtue, in the context of gravitation.[12]

The Dutch physicist Hugo Tetrode, also an acquaintance of Einstein, discussed the standing wave potential in a paper of 1922. At the time this solution to the classical wave equations seemed a possible explanation for the failure of orbiting atomic electrons to radiate. Furthermore, as Tetrode pointed out, in the quantum regime, the emission and absorption of radiation seemed to each depend on the other, rather than emission being required for absorption, but not the reverse. This suggested to him that the classical aversion to making absorption a requirement for emission should be discarded. As he put it, "The Sun would not shine if it were alone in the universe" (Tetrode 1922). In their paper, Einstein and Rosen appear to share Tetrode's preference for this potential, if not for his full action-at-a-distance program.

The story, in any case, is of particular concern to us, because of the project upon which Einstein and Infeld now embarked together with Banesh Hoffman. They wished to develop the post-Newtonian theory of the problem of motion, an ambitious project involving intensive calculations (Einstein, Infeld and Hoffman 1938). Since the non-linear field equations of relativity are too complex to be solved exactly for dynamical systems of masses, approximation schemes are required. In general relativity, two different schemes have been commonly employed. The post-Newtonian expansion makes corrections to the Newtonian motion of the system. Since the Newtonian limit is only valid for weak fields and slow motion, the expansion is in powers of the field strength and the source velocities. An alternative approach is to make corrections to the linearized equations of motion, in an expansion based on powers of the field strength alone. Because it was not limited to small velocities, the second approach became known as the fast motion approximation (and since the 1970s as "post-linear" or "post-Minkowski").

The problem of motion had been previously tackled by Einstein and others,[13] but the post-Newtonian Einstein-Infeld-Hoffman (EIH) method, was to be one of the more influential, in a very general way. Einstein particularly wished to vindicate his conjecture that in general relativity the allowed motions of the particles were completely determined by the field equations (Einstein and Grommer 1927), in contrast to other field theories where a separate force law is invoked.

Not long after the work was successfully completed, Infeld, who had with Robertson's help secured a position at the University of Toronto, put his graduate student Phillip Wallace to work applying the EIH formalism to the problem of motion in electrodynamics. In their paper, as also in the EIH paper itself (where radiation effects were not considered), we see a preference for the averaged potential, "half advanced plus half retarded". Infeld and Wallace state that this solution "does not specify a privileged direction for the flow of time" and is besides the simplest for their method (Infeld and Wallace 1940). They note that this solution does not damp orbital motion, and further state that "the addition of radiation seems from this point of view arbitrary", since one must choose the retarded potential to obtain it. This viewpoint partly reflects Einstein's own. The solutions which admit radiation damping are objectionable because they involve an arbitrary imposition of the arrow of time into field theories which are otherwise time-symmetric. Although Ritz had pointed out how this arbitrariness was an unsatisfactory feature of electrodynamics, his conclusion had been that one must choose the retarded potential to make any sense of it, until a theory which imposed it could be found. Einstein however, felt that time asymmetry had no business in field theories and that its origins lay solely in probability theory(Einstein and Ritz 1909). His views may have influenced Infeld, who preferred the "standing wave" solution as the most natural choice in the EIH approximation. In the case of the gravitational field, where the existence of radiation could not be experimentally proven, Infeld may have felt there was no compulsion to impose the arrow of time, as one would in electromagnetism, knowing from experiment that radiation existed in that field.

In the 1970s, Rosen returned to the problem of the arrow of time in gravitational radiation theory, in a paper whose title notably echoed that of his rejected 1936 submission to Physical Review with Einstein (Rosen 1979). In "Does Gravitational Radiation Exist?" he adapted the Wheeler-Feynman absorber theory to gravitation, and concluded that as the gravitational force interacted much less strongly with matter than the electromagnetic field, a source system would not undergo radiation reaction for lack of a sufficiently strong absorber field. In the Wheeler-Feynman theory it is the field of the absorbers, back-reacting on the source, which breaks the time symmetry of the source field. (Wheeler and Feynman, 1945 and 1949). However, Rosen's arguments do not appear completely convincing even to himself, since towards the end of the paper he retreats to a more Tetrode-like position, conceding that an absorber (such as a gravity wave detector) could presumably act so as to draw energy from the source at a distance. In any case, his paper did not excite much debate on the subject.

5 Post-War work

The first post-Newtonian attempts to deal with gravitational radiation reaction via the problem of motion had to wait until after the war. In 1946 Ning Hu, a Chinese graduate of Caltech, presented results based on a scheme inspired by the EIH method to the Royal Irish Academy in Dublin, reporting an energy loss disagreeing with the quadrupole formula in the case of an equal mass binary system in a circular orbit (Hu 1947). Shortly before publication, however, he added a note in proof after finding a calculational error which changed the sign of his result, giving anti-damping instead of damping. In other words, the system would gain, rather than lose energy as the result of emitting radiation. The binary would therefore slowly increase, not decrease in radius. In Canada, Infeld and his student, Adrian Scheidegger, worked on the problem of gravitational radiation reaction in the EIH formalism (Infeld and Scheidegger 1951). They concluded that the most natural treatment of the scheme, employing the standing wave boundary condition, led to a no-radiation-reaction result. It was possible, they conceded, to find terms at certain large odd powers of v/c (where c is the speed of light, and v represents the small source velocities) which appeared to correspond to back-reaction terms, but they contended that these could always be transformed away by a suitable choice of co-ordinates. The result, when announced at an American Physical Society meeting in 1950, "gave rise to a considerable flow of discussion", as Scheidegger put it (Scheidegger 1951). That same year Infeld left Canada, after a McCarthyite campaign against him organized in the press and in parliament, absurdly alleging that he was in possession of atomic secrets. He returned to his native Poland, while Scheidegger continued to argue the no-damping position in North America in his absence, before leaving the field of general relativity for that of geophysics in the mid-fifties.

In 1955 came two further contributions. Joshua Goldberg, a student of Peter Bergmann (who had criticized the Infeld and Scheidegger results), examined the reaction problem in the EIH formalism (Goldberg 1955). His conclusions were twofold. On the one hand, he denied that the slow motion approach tended to exclude the possibility of damping (arguing that co-ordinate transformations which removed some back-reaction terms, would reintroduce other reaction terms of odd order in v/c), but on the other hand, he determined that it was poorly suited to the back reaction problem, principally because of the restriction to slow motions of the source. In fact, it was generally agreed that radiation reaction terms did not enter into the post-Newtonian equations of motion until terms of order at least $(v/c)^5$ beyond Newtonian order (or post-$2\frac{1}{2}$-Newtonian order). Since first post-Newtonian effects (or $(v/c)^2$ order), such as those obtained by EIH, were both small and difficult to calculate, the expansion method seemed unpromising for studying radiation in that it had to be pushed to high order to succeed. A couple of years later Goldberg was introduced to Peter Havas, a physicist with experience in the problem of radiation in special relativity, who shared his interest in developing a fast motion expansion in general relativity. Having each worked on the problem independently, they began a collaboration based on this approach.[14]

Also in 1955, the Russian physicist Vladimir Fock treated the orbital damping problem in his book *Spacetime and Gravitation* (Fock 1959). He made use of a slow-motion expansion which he had developed independently of EIH, coupled with "no-ingoing wave" boundary conditions in the past of the system. His results were in agreement with those of Landau and Lifschitz. His work was not translated into English for four years, and even then wielded little influence in the west, perhaps because of Fock's unorthodox views on general covariance. He employed so-called harmonic co-ordinates in his calculations, and claimed a special status for them in physical theory. His views in this regard were vigorously opposed by Infeld and most other relativists then and since. Furthermore, Fock himself regarded his back-reaction result as merely demonstrating that wave phenomena played an inconsequential role in the problem of motion in gravity, due to the small size of the effect for known astronomical systems.

6 The Bern and Chapel Hill conferences

Between the war and the Bern conference of 1955 marking the 50th anniversary of special relativity, general relativity was at a low ebb (Eisenstaedt 1986a and 1986b). Work on the radiation problem seemed confused and controversial, leading only to some consensus that the problem required closer attention. At the Bern conference Rosen, returning to the cylindrical wave solution of his 1937 paper with Einstein, adduced evidence backing up Scheidegger's position by proposing the possibility that gravitational waves did not transport energy (Rosen 1955). It is a peculiar characteristic of general relativity that the energy contained in the gravitational field, and thus the energy in gravitational radiation, is not described in a coordinate invariant way. This energy is considered to be real enough, and can be converted into other forms of energy which can be expressed invariantly, but the principle of equivalence prevents one from doing this for field energy in gravity. The reason is that any observer in a gravitational field is always entitled to imagine himself in a locally Lorentz (that is zero gravity) freely falling frame of reference which, locally, contains no field energy. Of course, one is not free to transform away the entire field energy of a planet but one can always choose co-ordinates on a small portion of its surface so as to eliminate the field energy in that region. Thus it is said that gravitational field energy is non-localizable.[15] This problem of defining field-energy had led Einstein, Landau and Lifschitz and others to employ a non-invariant quantity known as a pseudo-tensor to describe energy in the wave flux in their back reaction calculations. Rosen now observed that each of these (slightly different) definitions of the pseudo-tensor showed no energy at all when applied to the cylindrical waves of his 1937 paper with Einstein in cylindrical co-ordinates. Although drawing conclusions on the tentative basis of the pseudo-tensor was regarded as dangerous, Rosen observed that the result seemed to support the view of Infeld and Scheidegger. This cast further doubt on the uncertain status of wave phenomena in gravitation theory.[16]

The Bern conference is remembered as an important stimulus to the field of relativity. The discussions there, and the interest taken by Felix Pirani, prompted Hermann Bondi to take up the problem of gravitational radiation.[17] Bondi brought an open mind to the issue, in the sense that he was sceptical enough of the existence of gravitational waves. He was influenced in this by Eddington, from whose writings he learned relativity. Eddington's emphasis on a coordinate invariant approach, making use of tensorial quatities such as the Riemann curvature tensor, had enabled him to show that certain classes of gravitational waves "in existence" before 1922 were spurious (Eddington 1922). Bondi, like some relativists of the day, was not impressed by the existing radiation reaction work, finding Landau and Lifschitz' treatment "a little glib".[18] At the same time, gravitational waves seemed like an attractive topic within gravitational theory, since in this area the predictions of general relativity diverged radically from those of Newtonian gravitational theory. Up to this time, most work in relativity, outside of cosmology, had been devoted to deriving small corrections to Newtonian theory, such as the famous perihelion shift of Mercury, a more precise estimation of which was one of the goals of the EIH paper (Robertson 1938). The study of gravitational waves, if they existed, seemed likely to generate more "new physics" than simply adding terms to Newton's theory.

Now, as Infeld himself observed when writing of his surprise at Einstein's "proof" that waves did not exist, no respectable modern field theorist would, under normal circumstances, deny the existence of radiation in a field theory. The mere fact that the force was propagated in the field rather than by action-at-a-distance, a basic tenet of all relativistic field theories, seemed to imply the existence of radiation. Einstein also remarked, in his letter to Born, of the "certainty" which the analogy between the linearized Einstein equations and electromagnetism had inspired concerning the existence of a gravitational analogue to the Maxwellian wave equation. Bondi nevertheless seized on a key argument made by Infeld and Scheidegger, which seemed to him crucial.

As Scheidegger observed, relativity occupied a "peculiar place" amongst classical field theories (Scheidegger 1953). One important peculiarity is that the equations of motion are constrained by the field equations, as Einstein had noted. In electrodynamics, where this was not the case, one was perfectly free to demonstrate damping effects by moving the particles around in whatever fashion, and showing that this gave rise, when the field equations were invoked, to radiation and loss of energy from the local system. In relativity, it was necessary to show that the motions in question were allowed by the same field equations. This was all the more important when one considered the question of what *type* of motion gave rise to radiation. One obvious example was an accelerating charge in electrodynamics. What of the apparently equivalent case of a falling mass? It was clearly accelerating with respect to the person who dropped it, but in a relativistic sense, it was merely following a geodesic, doing what came naturally, as it were.[19] In terms of the local spacetime, the particle that was really being *accelerated* was the one still being held in the observer's other hand, which was prevented from falling freely. Which one of these particles *ought* to radiate? This question had no immediately obvious answer which the relativists of the day could agree upon.[20]

At the Chapel Hill conference of 1957 and elsewhere at that time, Bondi pointed out the distinction between two masses being waved about at the end of someone's arms,[21] clearly not following geodesics, and clearly emitting gravitational waves (but infinitesimally weak ones!), and two masses in a binary star system, following geodesics and, if Infeld and Scheidegger were right, not radiating anything (De Witt 1957, pg. 33). Since gravitational forces were likely to be the only forces capable of moving large masses very quickly, the issue of whether purely gravitational systems could give rise to radiation was an issue of whether such radiation would ever be detectable. That issue, to the surprise of most theorists, was soon to become one of some practical interest.

The Chapel Hill conference on "The Role of Gravitation in Physics" brought together relativists and theoretical physicists interested in then new topics such as quantum gravity. The session on gravitational radiation was lively and varied. Felix Pirani presented important new work on wave theory (De Witt 1957, pg. 37). Influenced by the Irish relativist John Synge during a year spent in Dublin,[22] Pirani drew attention to the Riemann curvature tensor, whose importance had previously been stressed by Eddington in his 1922 paper, as an invariant geometrical quantity which was well suited to the description of the behavior of gravitational waves. Using the geodesic deviation description of gravitational effects advocated by Synge, he showed how particles in the path of a wave were moved about relative to each other by the spacetime curvature of the passing wave. In this view, gravitational waves were depicted as ripples in the fabric of spacetime itself, whose physical effects were observable by monitoring the relative motion of two adjacent particles during the passage of a wave.

Later in the conference an interesting exchange took place during the section on quantization of gravity. During Richard Feynman's presentation on the need for a quantum theory of gravity, Rosenfeld made the following remark:

It seems to me that the question of the existence and absorption of waves

is crucial for the question whether there is any meaning in quantizing gravitation. In electrodynamics the whole idea of quantization comes from the radiation field, and the only thing we know for sure how to quantize is the pure radiation field. (De Witt 1957, p. 141)

Feynman demurred somewhat from the premise, arguing that there existed a quantum theory of electrostatics, but agreed that some of his arguments in favor of quantization depended on the existence of waves. Bondi was moved to note that "this vexed question of the existence of gravitational waves does become more important for this reason." Feynman then presented an argument based on Pirani's earlier talk. Appealing to the equation of geodesic deviation, he argued that a particle lying beside a stick would be rubbed back and forth against the stick by a passing wave, and the friction would generate heat, so that energy would have been extracted from the wave. Furthermore, he felt that any system which could be an absorber of waves, could also be an emitter. For these reasons, he expected gravitational waves to exist (supplement to De Witt 1957).[23]

This line of argument, suggested by Pirani's new work, was also elaborated in two papers published that same year. In a letter to Nature, Bondi used a slightly different version of it to refute Rosen's argument of 1955 on energy transport (Bondi 1957), as did Joseph Weber and John Wheeler in a more detailed paper (Weber and Wheeler 1957). Weber demonstrated real confidence in the physicality of gravitational waves by embarking within a few years on an experimental program to detect them, using large resonant metal bars as antennae (Weber 1960). Quixotic is probably not quite the word contemporary theorists would have used to describe Weber's aim.[24] The wave theory, in so far as it existed at all, with no particular notion as to potential astrophysical sources or signals, would be better described as a "disabling" rather than an enabling theory for experiment. The quadrupole formula, the only guide to source strength and signal amplitude, suggested that any waves reaching the detector would be very weak. With no theory of sources, the question of what frequency to search at was theoretically undetermined.[25] It is remarkable that the field of gravity wave detection began at a time when the theoretical state of the subject was in such disarray.

7 The Rebirth of Relativity

An important requirement for the development of any scientific field is funding. The field of gravitational wave theory was fortunate in this regard in that, from 1956 to 1963, Joshua Goldberg was responsible for United States Air Force support of research in general relativity, based at the Aeronautical Research Lab at Wright-Patterson Air Force Base in Ohio. At this time, and up until the passage by Congress in 1972 of the Mansfield Amendment prohibiting the Department of Defense from sponsoring basic scientific research, the US armed forces provided considerable financial support for even very esoteric subjects in theoretical physics. Goldberg was active himself in the study of gravitational radiation, as we have seen, and did much to encourage groups such as that of

Bondi and Pirani at King's College, London. Although support was available for groups outside the US, it was not permitted to support scientists based in communist countries, inhibiting the use of these funds to facilitate travel between the London group and Infeld's group in Warsaw, who interacted extensively.[26] The Air Force laboratory itself was home to an active group until 1972. With one of his earliest grants, Goldberg was able to support the Chapel Hill conference organized by Bryce De Witt with Air Force money, and this important meeting became the forerunner of the successful General Relativity and Gravitation (GRG) series of conferences, which continues today. For a valuable account of this unlikely episode in the history of general relativity, see Goldberg (1988).

Following the Mansfield Amendment, research in relativity theory in the US depended primarily for its support on the National Science Foundation (NSF). From 1973 to the present, the chief advisor on funding for gravitation physics at the NSF has been Richard Isaacson, like Goldberg a relativist who has made important contributions to the theory of gravitational waves. Isaacson had also previously worked at the Air Force laboratory on the Wright-Patterson base. By good fortune then, despite the overall decrease in funding for theoretical physics precipitated by the Mansfield Amendment, the principal source of funds for research on gravitational wave theory remained in sympathetic and knowledgeable hands.[27]

As interest in relativity grew in the period after Chapel Hill, the reaction problem was pursued with renewed vigor. The EIH approximation was adopted by Andrzej Trautman, a student in Infeld's group in Warsaw, who departed from Infeld's approach in adopting "outgoing wave only" boundary conditions. He also confirmed Goldberg's earlier claim that the net back-reaction effect could not be transformed away, merely moved from one point in the expansion to another. He found positive damping, although differing somewhat from the quadrupole formula result (Trautman 1958a, 1958b). Infeld himself stuck to his earlier opinion, despite the contrary views of his students. In his 1960 book, *Motion and Relativity*, he included a detailed argument against the existence of back reaction in freely falling systems (Infeld and Plebanski 1960), without the knowledge or agreement of his co-author and former student, Jerzy Plebanski.[28] Another effort at this time, by Peres, initially found anti-damping, as had Hu, but this was corrected shortly after, and his new result agreed with that of Landau and Lifschitz for circular binary orbits (Peres 1959,1960). Peres' second paper has been been referred to as containing the first correct back reaction calculation (Thorne 1989). Nevertheless, the perceived arbitrariness of the slow-motion approach in imposing the wave zone boundary conditions from one step in the expansion to the next, which seemed reflected in the wildly differing results produced by the method, gave rise to arguments that the approach was hopeless (Bonnor 1963).

While conceptually more appealing in some ways, the alternative fast-motion approach, as developed by Havas and Goldberg (Havas and Goldberg 1962) and others (for example, Bertotti and Plebanski 1960), was also proving frustrating. It was a difficult task to go beyond the leading order corrections to the linearized theory and the results of applying that step to the reaction problem, published by Smith and Havas, again showed an energy gain in the source (Smith and Havas 1965). Therefore many theorists at the time concluded that the question of whether freely falling sources experienced damping remained unsettled.

Bondi, who with his collaborators had done much to improve the understanding of wave propagation far from the source (see especially Bondi, van der Burg and Metzner 1962 and Sachs 1962) made this point at the Warsaw conference of 1962 (Bondi 1962).[29] However, there were those, like Feynman, who viewed the relativists' caution with impatience. Feynman was "surprised to find a whole day at the conference devoted to this question" (of whether gravity waves could carry energy), as far back as Chapel Hill (letter from R.P. Feynman to Victor Weisskopf, February 11, 1961)[30], and was caustic in his appraisal of the discussions at the Warsaw conference, noting they were "not good for my blood pressure" in a letter to his wife (Feynman 1988). Bondi's lecture, however, inspired the astrophysicist Subrahmanyan Chandrasekhar to take up the problem.[31] Throughout the 1960s, Chandrasekhar developed his own slow-motion formalism, dealing with extended fluid bodies (as opposed to point masses) at one post-Newtonian order after another (Chandrasekhar 1965). By the end of the decade he had advanced far enough in the expansion (to post-$2\frac{1}{2}$-Newtonian order) to describe reaction effects. His conclusion agreed with the quadrupole formula result (Chandrasekhar and Esposito 1970). At about this time William Burke, a student of Kip Thorne's at Caltech, introduced improvements to the slow-motion approach which removed much of the arbitrariness in imposing the boundary conditions. Burke made use of the applied mathematics technique of matched asymptotic expansions, which allowed one to determine the solution to the problem of motion in the zone near the source, by matching it through an intermediate zone, to the "outgoing wave only", or other potential of choice, in the far zone of the waves. In this way the chosen boundary condition could be unambiguously applied to the solution of the near zone problem, thus addressing the arbitrariness which bedeviled the slow motion approach up to this time (Burke 1969). Employing Burke's novel approach, Burke and Thorne also derived the quadrupole formula for emission from binary systems (Burke and Thorne 1970).

During the sixties, great progress had been made on many fronts in the description of wave propagation and interaction with matter. Possible astrophysical sources, such as supernovae and binary neutron stars, began to be suggested, inspired at first by Weber's work (Dyson 1963).[32] Some experts were of the opinion that the subject was maturing and furthermore the prospect of some real astrophysical application for gravity waves, seemed to emerge with the discovery of the quasi-stellar ("quasar") radio sources (Fowler 1964; Robinson, Schild and Schucking 1965 and Cooperstock 1967). Then, to the great surprise of the theoreticians, Weber announced in 1969 that he was detecting gravitational waves (Weber 1969). Although his results, which confounded all theoretical predictions of source strengths then and since, were eventually discounted amidst much controversy, they focused much attention on the subject, and sparked a great increase in the number of experimentalists working on gravitational waves. (See Collins 1975 and 1981 for a detailed account, and Franklin 1994 for an alternative viewpoint). On the theoretical front, research in the 1960s on black holes, cosmology and other topics had made the field of relativity very relevant to astrophysics. Gravitational waves shared somewhat in this popularity, and seemed likely to continue to grow in practical importance as experimental interest waxed. The discovery of the first binary pulsar (PSR 1913+16) by Hulse and Taylor in 1975 (Hulse and Taylor 1975) crystalized the excitement in the field, providing the first test bed for strong field effects of general relativity, although there were doubts at first that the system would exhibit measurable orbital damping effects (Damour and Ruffini 1974).[33]

8 The Quadrupole Formula Controversy

The successes in improving the slow motion approximation, and the increasing likelihood of practical applications of gravitational radiation theory encouraged some experts, such as Kip Thorne, to suggest that the reaction problem was now well understood,[34] and the multipole formalism could be used with confidence in astrophysical applications to give approximate estimates of source strength, much as one would in electromagnetic wave theory (Thorne 1980). This viewpoint however, was sharply opposed by some others who were still seriously dissatisfied with the state of the field.[35] They were particularly concerned that the quadrupole formula would be used as a reliable formula in contexts in which its results might be wholly misleading. One of these was Havas who was still very unhappy with the various slow motion results (Havas 1973).[36] One of his students, Arnold Rosenblum, brought the unsatisfactory state of affairs to the attention of the mathematical physicist Jürgen Ehlers, who also took up the cause of alerting the relativity community to the dangers of complacency on the matter.[37]

The alarm sounded by Havas, Rosenblum and Ehlers had the effect of again focusing attention on the reaction question, and this interest was redoubled by the announcement, in 1980 of observations of orbital decay in the binary pulsar. Taylor and coworkers, after years of careful observation of the system, were able to announce an orbital period decrease in line with the predictions of the quadrupole formula with an accuracy of measurement of about 20% (Taylor and McCulloch 1980). The warnings of the unverifiability of the quadrupole formula within the theory now had their effect. A chief use of the binary pulsar data since its discovery had been as a test of general relativity against rival theories of gravity. Agreement between observation and the quadrupole formula could only constitute a test of general relativity theory if the quadrupole formula was established as a prediction of the theory. Not all relativists were of the opinion that it was so established.[38] There was a surge in interest in the problem of motion and in back-reaction in particular, including by some who had not previously worked on the problem. The great majority of the new results vindicated the use of the formula. A further round of sharp debate ensued as these results, via a wider variety of approaches then ever before, and in greater detail than ever before, convinced many that the issue was at last settled, pushing the remaining sceptics into an embattled minority. As the eighties advanced, and the increasingly convincing experimental data continued to agree solidly with the theoretical work carried out in close parallel by Thibault Damour and his collaborators

(Damour 1983), the debate slowly died away. At present the focus in the field is on calculating higher order contributions to the waveforms produced by binary systems for use in conjunction with data extraction techniques in the next generation of wave detectors, indicating a high degree of confidence in most quarters in the basic theoretical position.

An important feature of the radiation reaction debate in the seventies and eighties was the series of review papers by different authors, each employing the history of the subject to illustrate a particular view of the contemporary state of the field. These papers show that relativists were keenly aware of the history of their field and they were able to draw lessons from their reading of history which reinforced the points they wished to make. The earliest of these papers was that of Ehlers, Rosenblum, Goldberg and Havas whose argument was that previous attempts to deal with the back-reaction problem were all inadequate in one way or another. In consequence, they advanced an outline of a program which would overcome these past failings (Ehlers, Rosenblum, Goldberg and Havas, 1976). Essentially an attempt to formulate a research program for the subject, their paper was followed by an Enrico Fermi summer school in Varenna organized by Ehlers, whose aim was also to foster new work in the field along more rigorous lines than before (Ehlers 1979).

Walker and Will in 1980 took a very different tack, addressing the problem of non-reproducibility which had plagued the subject (Walker and Will 1980). They argued that a basic iterative algorithm, applicable for both fast motion and slow motion methods, could be followed to recover the quadrupole formula from reaction calculations. They presented an analysis of a cross section of well-known calculations, dating back to the paper of Hu in 1947, and argued that those which had advanced through sufficient steps in the iteration recovered the quadrupole formula, and that others, with fewer steps did not (except for a couple which found the result with the aid of compensating errors). In this view of the history of the field, there existed a definitive method by which the standard results could be recovered in a reliable way. This was in stark contrast to the views expressed by Ehlers et al., which were to advocate a more general prescription, whose outcome was not yet known. Yet another view was put forward by Cooperstock and Hobill in 1982. They refused to set forward a general scheme or advocate a particular result, instead arguing against preconceived notions (Cooperstock and Hobill 1982). Their history, as befitted their standpoint, was more descriptive than prescriptive, celebrating the diversity in the development of the field. Another protagonist with an interest in and excellent knowledge of the field's history was Damour. His papers were often prefaced with a discussion setting his work in a historical context (for example Damour 1982). In this role, the object of history was to motivate the new work being presented, and the focus was on the previous failings which were being addressed by the new contributions (see, for instance, Damour 1983). A more active role for the historical literature was found in the account of James Anderson, who returned to the Einstein-Infeld-Hoffmann scheme complete with its surface integral method, and married it to the matched asymptotic expansions of Burke, with further additions of his own, to produce another influential derivation of the quadrupole formula (Anderson 1987).

A very significant aspect of the debate in the seventies and eighties was the problem of when theory ends.[39] As we have seen, different authors could look at the same history and give very different answers to this question. One answer might be, it already has ended, we really know the answer ("Conservative"). Another is, it has just ended now, with this paper, for the issues addressed ("Technocratic"). A third is, it will end, as soon as the general program we advance is carried through ("Marxist"). A fourth is that it can never end, and it is best that it should not ("Anarchist"). Finally there is the view that the answer is hidden in the past, waiting to be extracted and pieced together from the literature ("Archaeological"). It is interesting that just as there was agreement on the details of the history (and the debate was largely a historical debate), opinions diverged on the matter of *interpretation.* The lesson of history was different for everyone. This is still the case, but the debate having lost its impetus, the individual perception of history has lost its public relevance once more. The dynamic of the debate is that some level of consensus must be found for the resolution of an existing problem, and yet progress seems to be measured by many scientists by the extent to which an issue can be settled, allowing the next problem to be addressed. A field like General Relativity has historical memories of the isolation which may be the fate of a discipline which does not progress in this way. The remarks of Feynman at Chapel Hill (De Witt 1957, pg. 150), express the view of the progressives, when he says "the second choice of action is to ... drive on," to "make up your mind [whether gravitational radiation exists] and calculate without rigor in an exploratory way". He concludes with the advice, "don't be so rigorous or you will not succeed."[40] The contrast in attitude suggested here may explain why the debate tended to become more vitriolic in its last stages, as a consensus developed for many, with some still arguing that the matter was unsettled.[41]

In studying the controversy following Weber's announcement of gravitational wave detections, Harry Collins (1985) has introduced the concept of the Experimenter's Regress. This describes the difficulty faced by experimenters when confronted with a dispute over non-confirmation of claimed results. Since none of the experiments will exactly duplicate the others' behavior, achieving consensus is hampered by the problem that the device which is working properly should get the correct result, but the correct result can only be known from the output of a properly operating device. Although Collins' view has been criticized (Franklin 1994), it seems to provide a useful model for understanding the Weber controversy. In the theoretical controversy surrounding gravitational waves, one seems to observe a similar phenomenon, the "Theoretician's Regress". The complex, tedious calculations designed to approximate to the full general relativity theory can be thought of as experiments, with the theory itself in the role of a notional "reality". These experiments constituted a delicate technical apparatus, designed to probe this "reality", aided by the craft and mathematical skill of the theorists. "Experimental error" was impossible to account for fully, whether as systematic error in the form of an inappropriate expansion scheme or failure to properly control errors from neglected terms (a difficult problem which was rarely addressed programmatically), or as accidental error in the form of simple calculational mistakes amidst the welter of terms which had to be collected.

As with the experimentalists, direct replication of another method was rarely even attempted. Even the best known schemes, such as EIH, were employed with improvements designed to simplify the calculations or overcome objections in principle, such as the use of point mass sources (Anderson 1995). Therefore, the array of review papers, conference workshops and other social efforts to achieve consensus had to overcome the cycle of regression constructed by the fact that the right scheme would be the one which gave the right result, but the right result was the answer given by the right scheme. The difference in emphasis between those who gave weight to having the right answer, and those who preferred to rely on method alone gave rise to further disagreement. One event which helped to partially break the cycle was the advent of the binary pulsar data. Initially this gave rise to more activity and more disagreement, but it also lent outside support to the preferred "right result" given by the quadrupole formula. It did not however, put an end to disagreements about the correctness of various methods, except in so far as it tended to rule out methods which disagreed with the canonical result. This was enough to gradually bring an to end the public side of the quadrupole formula controversy.

Throughout all this, one notes the tensions within the field over technical matters, especially regarding the level of rigor required to inspire confidence in a particular result. Relativity has a tradition which places it towards the mathematical end of the spectrum in this regard amongst branches of theoretical physics. Yet from the sixties on, astrophysics and relativity became relevant to each other, even spawning the new field of relativistic astrophysics. Theoretical astrophysics stands at the opposite extreme from relativity, preferring a more "physical" approach, eschewing not only mathematical rigor, but also dependence on exact results. Order of magnitude calculations and heuristic arguments are common. Such arguments, for instance, might be used to identify the "correct" result, as a guide when undertaking longer calculations.[42] Within relativity there were those whose practice tended towards each approach, and it was naturally difficult for them to agree on the question of standards of proof.[43] For practical purposes results such as the binary pulsar measurements were obviously welcome, but at issue was on whose terms a given result was to be accounted a prediction of general relativity: the "astrophysicists" or the "mathematicians".

A second, less significant, mixing of fields concerned attempts to quantize gravity. Especially in the fifties, it was argued by some that the existence of radiation was a crucial matter for this project (Rosenfeld in De Witt 1957, pg. 141; Rosen 1979). In fact, Felix Pirani's view was that "the primary motivation for the study of [gravitational radiation] theory is to prepare for quantization of the gravitational field." (Trautman, Pirani and Bondi 1965, pg. 368). The uncertain position of general relativity as an independent, yet thriving field, seems to have played into fears and attitudes concerning the radiation problem. Relativists' own practices and their own opinions of what the problems were in the field may have seemed endangered by the twin possibilities of classical relativity becoming a mere adjunct to astrophysics, and the theory as a whole being submerged by a unified quantum field theory of gravity (Roger Penrose in Lightman and Brawer 1990, p. 429). The emergence of relativity into the mainstream of physics had a highly ambivalent aspect for relativists, in that it brought with it the danger that the character of the small stream would be lost in the larger current. The fears and hopes which this dual prospect raised for scientists who had consciously chosen the field for its own beauty and intimacy no doubt helped shape attitudes in the debate.

9 A Final Note

This paper is an expanded version of a talk given at the fourth international conference on the History of General Relativity, held in Berlin, August 1-4, 1995. I have concentrated on the emergence of certain important issues which contributed to the uncertainty and controversy which at times surrounded the theoretical development of the subject of gravitational radiation. In doing so, not only have I focused here on the immediate post-war period up to about 1960, but I have deliberately not attempted to cover the entire breadth of the literature for any period. I have tried to illustrate how the debate on the existence of gravitational waves came to arise as a serious discussion, and how the subject's own history was used as a rhetorical and motivational tool in subsequent debates after its emergence as the subject of experimental and not just theoretical research. I have left many interesting aspects of the history of this problem for another time. For now, I have tried to give a sense of how the thought that gravitational waves might NOT exist first arose, and then became a serious issue in the field of relativity, and how the issues raised fed into later debate as the subject matured. It is worth noting that *scepticism* about their existence encouraged important scientists, such as Bondi, to focus attention on gravitational waves, at a time when those who were certain of their existence dismissed their effects as insignificant (Landau and Lifschitz and Fock, for instance). If the attempt to detect gravitational radiation is now a multi-million dollar field, thanks to the pioneering work of Weber on the experimental side, some credit must go to those who thought the theory of the subject worth advancing for reasons of principle many decades ago.

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archival material and conduct interviews. Finally, for their kindness, hospitality and patience, I thank all of those who were interviewed by me. They all helped to make this work a personally rewarding and enjoyable experience.

11 Notes

[1] The construction of history as part of the self-definition of a field of science is an important topic in the history of science. For an excellent discussion in a different context, see Barkan (1992).

[2] Although the original version of Einstein and Rosen's paper probably no longer exists, its original title is referred to in the report by the *Review*'s referee (EA 19-090).

[3] The translation from the original German is by Diana Barkan. The emphasis in the letter is Einstein's.

[4] In a letter to Einstein in March 1936, Cornelius Lanczos remarks on "the rigorous criticism common for American journals", such as the *Physical Review* (translated and quoted in Havas 1993, pg. 112). Infeld claims that the German attitude, by contrast, was "better a wrong paper than no paper at all." (Infeld 1941, pg. 190). Jungnickel and McCormmach (1986) describe the editorial workings of the *Annalen der Physik* in the first decade of this century in some detail. They note that "the rejection rate of the journal was remarkably low, no higher than five or ten percent", and describe the editors' reluctance to reject papers from established physicists (pg. 310). As this was the time and place in which Einstein began his published career, the "rigorous criticism" he was to experience very shortly after receiving Lanczos' letter must have come as something of a shock.

[5] Einstein's bibliography to 1949, given in Schilpp (1949) lists no papers by him appearing in the *Review* after 1936, and the index of the *Physical Review* from then until his death refers only to one short note of rebuttal, mentioned by Pais (1982) in his brief account of the rejection of the Einstein-Rosen paper.

[6] The paper appeared in the Franklin Journal under a different title and with radically altered conclusions in early 1937. That it had previously been accepted in its original form is indicated by a letter from Einstein to its editor on 13/11/36 (EA 20-217), explaining why "fundamental" changes in the paper were required because the "consequences" of the equations derived in the paper had previously been incorrectly inferred.

[7] Curiously, Infeld states that when he communicated to Einstein his discovery with Robertson of an error in his (Infeld's) version of the proof, Einstein replied that he had coincidentally and independently uncovered a (more subtle) error in his own proof the night before (Infeld 1941, pg. 245). He does tell us that Einstein's position still had to evolve from that of demolishing his proof, to that of reversing it (by showing an exact

solution for cylindrical waves), and this was Robertson's key contribution according to Rosen's paper of 1955. Unfortunately, Infeld gives us no details of the false proofs and their correction in his account, which was intended for a popular audience. He does relate the amusing detail that Einstein was due to give a lecture in Princeton on his new "result", just one day after completely reversing his conclusions on its validity. He was forced to lecture on the invalidity of his proof, concluding by stating that he did not know whether gravitational waves existed or not (Infeld 1941, pg. 246).

[8] The identity of the Review's referee is unfortunately not known. Few records of the journal exist for this period, and the report has only survived amongst Einstein's own papers. It is 10 pages long and shows an excellent, if not perfect, familiarity with the literature on gravitational waves (the referee knew of Baldwin and Jeffrey's 1926 paper, but not Beck's of 1925). The copy forwarded to Einstein is typewritten and the spelling follows American practice ("behavior" rather than "behaviour", "neighborhood" rather than "neighbourhood"). It is likely, therefore, that the author was an American with a strong interest in general relativity, not a very inclusive category at this time. It is tempting to suspect Robertson himself, but there is nothing to support this in his surviving (and extensive) correspondence with Tate.

[9] Interviews by the author with Hermann Bondi (November 7, 1994) and Felix Pirani (October 25, 1994). Pirani reviewed the McVittie (1955) paper for *Mathematical Reviews* and was dissatisfied with its conclusions (Pirani, 1955).

[10] In their work, Bondi, Pirani and Robinson followed the new approach of Lichnerowicz in imposing regularity conditions on the metric (Lichnerowicz 1955). For a thorough review of the tangled history of plane gravitational waves, see Schwimming (1980).

[11] See also Damour (1982) for a brief but interesting discussion of Laplace's "radiation reaction" calculation. It is now known, from laser range finding, that the moon is receding from the earth, not approaching it. But the increased lunar orbital angular momentum is gained at the expense of earth's rotational velocity, by tidal coupling. The resultant lengthening of the earth's day gives the appearance of quickening to all celestial motions, including the lunar orbital period (i.e. although the month has lengthened, it is shorter in terms of days, since the day has also grown longer).

[12] Since general relativity is a non-linear theory, the fact that two potentials (the advance and retarded) satisfy the field equations does not imply that their linear combination (half advanced plus half retarded) would, as it does in electromagnetism. In linearized gravity, however, this obviously does follow.

[13] See Havas (1989) for an excellent review.

[14] Interviews with Joshua Goldberg (April 10, 1995) and Peter Havas (April 5, 1995).

[15] In 1968, Richard Isaacson discovered an invariant tensorial quantity which described wave energy in a local sense, by averaging over a wavelength of the wave. Thus, using

this approach, gravitational wave energy can be localized within a wavelength, but no further (Isaacson 1968).

[16] As we shall see, Rosen's paper was soon answered in a manner convincing to most relativists. He himself revised his opinion on this matter in a letter to the Physical Review (Rosen 1958), after realising that, when using Cartesian co-ordinates, the pseudo-tensor did show energy in the cylindrical waves. His new calculations on the energy content of cylindrical waves did not appear until after some delay (Rosen and Virbhadra 1993). The issue was addressed in some depth in the fifties, however (Stachel 1959). The problem of the pseudo-tensor in the study of gravitational waves was not new then, nor has it entirely ceased to be the subject of debate since. In recent years, Fred Cooperstock has suggested that, based on the hypothesis that the preferred frames of reference when describing the field energy should be those which eliminate the pseudo-tensor, the gravitational field energy should be described only by an invariant tensor quantity. The result of this would be that the conservation relation in relativity would require that no field energy be present where there was no matter, preventing gravitational waves from propagating energy through empty space (Cooperstock 1992). In the very early days of general relativity Levi-Civita made a proposal with somewhat similar (but more drastic) consequences, in response to the confusing and incorrect results derived by Einstein in his 1916 paper on gravitational waves (Levi-Civita 1917). For a very interesting discussion of this episode, which includes some revealing comments reflecting the initial unease about gravitational radiation brought on by Einstein's early errors (including the mistaken conclusion of his 1916 paper that spherically symmetric motions of matter could generate gravitational waves), see Cattani and De Maria (1993).

[17] Interviews by the author with Felix Pirani (October 25, 1994) and Hermann Bondi (November 7, 1994).

[18] Interview with Hermann Bondi (November 7, 1994).

[19] The question of whether particles following geodesics should radiate, given that they are behaving "naturally" in a gravitational field, seems intriguingly Aristotelian.

[20] Interview with E.T. Newman (April 11, 1995). He relates how J.A. Wheeler once asked a roomful of relativists to vote on the answer to the two particle question and recalls the room being fairly equally divided. This seems to be a rare example of the "Democratic" approach to science.

[21] A number of those interviewed by the author recalled Bondi vigorously demonstrating this method of generating gravitational waves.

[22] Interview with Felix Pirani (October 25, 1994).

[23] Cooperstock's 1992 paper (see note 10) contains an argument based on a counter-example to the Feynman-Bondi thought experiments, which claims that no energy is deposited in the "absorber" despite the motion locally induced by the wave. His hypoth-

esis would imply that waves exist in general relativity, are detectable by certain types of instruments, but carry no energy. However, this paper and its conclusions have provoked little debate. This perhaps reflects the difficulty in physics of reopening an argument considered closed by most in the field. At some point, the premise of the paper becomes sufficient grounds for dismissal. However, the problem may be simply due to the fact that papers outside the current thrust of research interests are unlikely to receive much attention, whatever their conclusions.

[24] Several interviews (especially one with Joseph Weber June 20, 1995) and anecdotal recollections, as well as the impression given by conference proceedings, agree that the reaction to Weber's initial efforts to detect gravitational waves in the 1960s ranged between polite scepticism and derision.

[25] Interview with Joseph Weber (June 20, 1995).

[26] Interview with Felix Pirani (October 25, 1995).

[27] The advantage of having an insider at the primary funding agency did not ensure that everyone in the field was sponsored to the extent that they desired or felt necessary. Complaints about the funding choices made and its effect on research directions were very noticeable on the experimental side, where groups and research programs depended very heavily on the munificence of different (usually governmental) funding agencies. But even on the theoretical side, work on the problem of motion or radiation reaction was computationally so intensive that funding for postdocs and assistants could make a big difference to a group or research program. It may be that less popular research programs suffered in this regard (such as fast motion approximations versus slow motion ones), but it is difficult to assess the extent of this factor. This partial assessment is based on interviews by the author with Richard Isaacson (April 7, 1995), Joshua Goldberg (April 10, 1995), Peter Havas (April 5, 1995) and Joseph Weber (June 20, 1995). Given the importance of debates during conference sessions, it is also worth noting the complaint that, because of the influence of slow motion advocates such as Infeld on the organizing committee, the fast motion approximation was not discussed at any of the GRG conferences, such as Warsaw 1962 (Peter Havas, private communication). Thus, the growth of this research program may have been retarded by a lack of exposure.

[28] Interview with Jerzy Plebanski (June 30, 1995). In general, however, Infeld proved reasonably tolerant of the opposing viewpoints within his group. Indeed, in the late sixties, shortly before his death, he was finally won over by his students' arguments (interview with Andrzej Trautman October 17, 1994).

[29] Bondi, van der Burg and Metzner (1962) and Sachs (1962) showed that when a certain function (known as the Bondi news function) was present, an isolated system would lose mass to the emission of gravitational waves. At Warsaw in 1962, in the discussion with Bergmann and Feynman (Bondi 1962), Bondi stresses the importance of dealing with specific equations of state in the components of the binary system, because it was his opinion that binaries composed entirely of pressure free dust would not radiate,

as all particles would follow geodesics and there would be no possibility of "news", in the form of a departure from geodesic motion. In the case of a real physical system, even if no deviation from geodesic motion occurs, this is news, since no news, if not good news, is still news, if news was expected. Bondi eventually decided against his position that idealized dust filled binaries might not radiate (interview with Bondi November 7, 1995). In the same discussion in the Warsaw proceedings (Bondi 1962) Feynman gives a brief account of his own unpublished calculations which convinced him that gravity waves exist.

[30] A copy of this letter was kindly supplied to the author by Kip Thorne. Copies are also kept amongst the Feynman papers at Caltech.

[31] Interview with Subrahmanyan Chandrasekhar (July 12, 1995).

[32] Interview with Joseph Weber (June 20, 1995). Weber recalls that Freeman Dyson suggested asymmetric collapse of stars during supernova events as one possible source for his detectors in the early 1960s.

[33] Interview with Thibault Damour (October 11, 1994).

[34] Interview with Kip Thorne (July 17, 1995). Thorne recalls first putting this view forward at a meeting in Paris, June, 1967.

[35] Interview with Kip Thorne (July 17, 1995). He recalls Havas taking issue with his comments at the Paris meeting, June 1967.

[36] The non-linearities still continued to bedevil the problem in some people's minds. At Caltech, despite Thorne's complacency, Burke noted in early versions of his work that his approach was not guaranteed to work outside of linearizable systems, and therefore could not settle the issue for freely gravitating systems. There is still on display at Caltech the record of a wager between Burke and Thorne on whether non-linear effects would "significantly affect the radiation in the lowest order" from sources in free-fall motion. Thorne gave odds of 25-1 for this bet, which Burke conceded in 1970.

[37] Interview with Jürgen Ehlers (October 14, 1995).

[38] Interview with James Anderson (April 3, 1995).

[39] The analogy to the problem of *How Experiments End* (Galison 1987) should be obvious.

[40] The alert reader will have guessed that I have just described as "progressives" the same class of people whose historical outlook I earlier labeled "conservative". In this case, conserving and defending the orthodox historical account plays a crucial role in the progressive agenda, discouraging debate on topics which are regarded as settled and directing energy towards problem solving work within the established paradigm.

[41] Interview with Fred Cooperstock (June 26, 1995). A number of other interviewees recalled rather heated exchanges taking place at conferences in the early 1980s during the quadrupole formula controversy.

[42] This question of "style" in physics seems an important one. Chandrasekhar suggests that the greatest physicists (such as Newton) employed both of these styles equally well. He relates that Fermi would say that he would not believe a physical argument without a mathematical derivation, nor would he believe the mathematics without a physical explanation. Interview with S. Chandrasekhar (July 12, 1995).

[43] Interviews with Kip Thorne (June 14, 1995) and Jürgen Ehlers (October 14, 1995).

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